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Pricing accuracy, liquidity and trader behavior with closing price manipulation

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Abstract

We study the effects of closing price manipulation in an experimental market to evaluate the social harm caused by manipulation. We find that manipulators, given incentives similar to many actual manipulation cases, decrease price accuracy and liquidity. The mere possibility of manipulation alters market participants' behavior, leading to reduced liquidity. We find evidence that ordinary traders attempt to profitably counteract manipulation. This study provides examples of the strategies employed by manipulators, illustrates how these strategies change in the presence of detection penalties and assesses the ability of market participants to identify manipulation.

Keywords: manipulation, closing price, high-closing, experimental market

JEL Classification: G14, C90

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1. Introduction

Two fundamentally important aspects of financial market quality are pricing accuracy and liquidity. Pricing accuracy, the precision with which market prices reflect the underlying value of an asset, determines the informativeness of prices and their ability to encourage efficient resource allocation. Liquidity allows efficient transfer of risk. The presence of traders with incentives to manipulate prices is a feature of markets that may limit their informational and transactional efficiency.

The central aim of this paper is to identify how closing price manipulation affects pricing accuracy and liquidity in order to evaluate manipulation's social harm. In their discussion of how to define illegal market manipulation, Kyle and Viswanathan (2008) argue that forms of manipulation should only be illegal if they are detrimental to *both* pricing accuracy and liquidity. Their argument is based on the premise that if a manipulator distorts pricing accuracy but brings about greater liquidity, or vice versa, depending on the relative social value of these two externalities, it may be economically efficient to allow such forms of manipulation.

Empirical examination of these issues is difficult. Anecdotal evidence suggests prosecuted manipulation is a small proportion of all manipulation and is systematically different to undetected or not prosecuted manipulation. This causes biases in empirical analyses that cannot be corrected with conventional approaches such as Heckman estimators or instrumental variables due to the nature of this partial observability problem. Further, 'true' asset values, incentives and information sets, as well as counterfactuals such as manipulation free markets, are generally not observable. We therefore study closing price manipulation in an experimental market.

We find that manipulators, given incentives similar to many actual manipulation cases, decrease price accuracy (ex-post) and liquidity (ex-post and ex-ante). The mere possibility of manipulation alters market participants' behavior causing reduced liquidity. We find some evidence that ordinary traders attempt to profitably counteract manipulation. However, in our experimental market their effect is not strong enough to prevent the harm caused by manipulation. Finally, this study provides examples of the strategies employed by manipulators, illustrates how these strategies change in the presence of detection penalties and assesses the ability of market participants to identify manipulation.

From the various forms of market manipulation we focus specifically on closing price manipulation due to the large number of contracts that are based on closing prices and their widespread use by investors, financial managers and academics. Their widespread use provides many parties with incentives to manipulate closing prices. For example, mutual fund net asset values (NAV) and fund performance are often calculated using closing prices. Fund managers working for RT Capital Management Inc., a Canadian investment company with $34 billion under management, intentionally engaged in 53 instances of closing price manipulation during 1998 and 1999. The manipulation increased the reported value of their portfolio by more than $38 million and resulted in the collection of more management fees (which were a fixed percentage of the market value of managed assets) and greater remuneration for the portfolio managers.

As another example, the price at which seasoned equity issues and corporate acquisitions occur is often determined by closing prices. In 1999 Southern Union Company and Pennsylvania Enterprises entered into a merger agreement with the acquisition price determined by the average closing price over a ten-day period. Baron Capital Inc., a broker-dealer with $8.6 billion under management, manipulated closing prices during this period to benefit clients that had a substantial stake in the acquiring company. Baron Capital made the closing trade on seven of the ten days and accounted for 78% of the volume in that period. Closing prices have also been manipulated by market participants with positions in cash settled derivatives at expiry, brokers attempting to alter their customers' inference of their execution ability, to avoid margin calls, to maintain a stock's listing on an exchange with minimum price requirements and on stock index rebalancing days for a stock to gain inclusion in an index.

2. Related literature

The small body of existing evidence on the effects of market manipulation is mixed and inconclusive, largely due to the difficulties in empirically studying manipulation.

There is little doubt that manipulators are able to influence prices.[1] However, it is not clear how consistently and to what extent manipulators distort prices. Rational expectations theory predicts that if market participants are able to recognize manipulation they should profitably counteract it, thereby offsetting any price distortion. This intuition is central to the microstructure model in Hanson and Oprea (2009) in which manipulation causes prices to be *more* accurate due to increased liquidity from profit seeking investors. In contrast, other studies argue that manipulation reduces participation in markets resulting in lower liquidity, higher trading costs and higher costs of capital (see, e.g., Prichard, 2003).

Laboratory and field experiments provide further evidence of manipulation attempts that do not impair pricing accuracy. In an experimental market involving asset trading via an electronic limit order book, Hanson et al. (2006) find no evidence that manipulators are able to distort prices. In a field experiment involving attempts to manipulate horse racing odds, Camerer (1998) reports that manipulation failed to distort prices.

A further issue is how regulation, i.e., a system that imposes penalties on detected manipulators, affects manipulators' strategies, pricing accuracy and liquidity. In an inter-jurisdiction study, Cumming and Johan (2007) find that more detailed market manipulation rules increase trading activity through enhanced investor confidence. Bhattacharya and Daouk (2002) find in a sample of 103 countries that the *enforcement* of laws governing financial conduct, rather than simply their presence, affects markets in a positive way. Little is known about how manipulation strategies change in response to regulation.

Hanson et al. (2006) conduct the first laboratory work on price manipulation in asset markets. Their main result is that manipulators are unable to distort price accuracy throughout trading sessions because other traders counteract the manipulators' actions. We extend Hanson et al. (2006) in several important ways. First, we consider not only

[1] There is direct empirical evidence in Aggarwal and Wu (2006) and Comerton-Forde and Putniņš (2010), indirect empirical evidence in Carhart et al. (2002), Hillion and Suominen (2004), Khwaja and Mian (2005), Ni et al. (2005) and evidence from theoretical analyses in Allen and Gale (1992) and Kumar and Seppi (1992).

pricing accuracy, but also the effect of manipulation on liquidity – the second externality that must be understood to draw conclusions about manipulation's social harm or benefit. Second, by making the presence of manipulators uncertain we are able to examine how the possibility of manipulation alters trading characteristics (ex-ante effects). Third, we examine how regulation affects manipulators' strategies and other traders' reactions. Finally, and most importantly, we examine a different form of manipulation - closing price manipulation - by giving manipulators incentive to realize high *closing* prices as opposed to high prices throughout a trading session. This last difference is critical in determining how manipulation affects markets.

Closing price manipulation is arguably easier to carry out because the manipulator needs only to sustain a liquidity imbalance for a short time period just prior to the close. A typical example involves aggressive buying or selling in the final moments of trading. Consistent with this, empirical evidence indicates that the price distortions caused by closing price manipulation are reversed the following morning (Carhart et al., 2002; Comerton-Forde and Putniņš, 2010). In contrast, trading to maintain an artificially inflated or deflated price for a longer period of time is more costly. Consequently, manipulators of intraday prices typically use different strategies such as rumors, wash sales and attempts to corner the market.

3. Experiment design and procedure

Our experiment consists of three treatments: a control with no manipulators, a treatment to examine the ex-ante and ex-post effects of manipulation and a treatment to examine the effects of regulation. In all treatments 12 subjects trade shares of a common asset in an electronic continuous double auction market.[2] Each experimental session consists of 16 trading periods of 200 seconds each, under one of the treatments.

Treatment 1 replicates a variation of a classic design developed by Plott and Sunder (1988) to study information aggregation, and is similar to the control treatment

[2] Forsythe and Lundholm (1990) examine the effect of the number of traders in a similar experimental market and find that 12 traders is a suitable number for competition among traders to drive the market to perform as predicted by a rational expectations model. Hanson et al. (2006) also use 12 traders in their experimental markets.

used by Hanson et al. (2006). The fundamental value of the asset, V, is unknown to individual subjects during the course of trading and is revealed at the end of each period. However, it is made common knowledge among subjects that $V \in \{20,40,80\}$ with an equal probability of each value occurring. At the start of each trading period subjects are endowed with four shares of the common asset, 200 experimental currency units (ECU) and a clue about V. The clue is knowledge of one of the three possible values that V will certainly not take in that period. For example, if $V = 40$, half the traders (chosen at random) are told $V \neq 80$ and the other half are told $V \neq 20$. Although no individual knows the true fundamental value, V, in aggregate subjects have enough information to determine V.

At the end of each period the shares owned by participants are converted to cash at their fundamental value, V, and, together with any remaining cash, added to the traders' payoff pools. The traders' payoff pools determine how much they are paid for participating in the experiment, as explained later. Traders' endowments are reset to the original amount of four shares and 200 ECU at the beginning of each period.

Treatment 2 introduces the possibility of manipulation by giving some subjects incentives to manipulate the closing price. In half of the trading periods (randomly selected) a trader drawn at random is informed that they will assume the role of manipulator for that period. The remaining traders, from the beginning of the experimental session, are aware that manipulators will be chosen at random in some periods, but they do not know which periods or traders.

Manipulators receive the same initial endowment as other traders (including the clue about V) but different payoffs. A manipulator's payoff is $15(P_{closing} - P_{median}) + 250$, where $P_{closing}$ and P_{median} are the closing price (last traded price) and median price, respectively. This payoff provides incentive for manipulators to try and increase the last trade price irrespective of V. The median price is chosen as the reference point for calculating manipulation profits because it is difficult to manipulate (as demonstrated by Hanson et al. (2006)) and is consistent with many real examples in which manipulation profits are a function of closing prices relative to prevailing intraday market prices.[3] Unlike several other forms of market manipulation, closing price manipulators often profit from sources external to the market, such as overstated fund performance. This is simulated by the payoff we provide to manipulators. Periods with a manipulator allow us to examine ex-post effects of manipulation and periods without a manipulator provide evidence on the ex-ante effects of manipulation (the effect of possible manipulation).

At the end of each period ordinary traders submit guesses as to whether a manipulator was present in the market. Guessing whether the 'manipulator' was present is equivalent to guessing whether market manipulation, in the legal sense, occurred. This is because as long as subjects attempt to maximize their payoffs, the manipulator's payoff ensures that they will intentionally attempt to alter the market price, and such actions constitute a violation of securities laws in most jurisdictions. Correct (incorrect) guesses earn (cost) the subject 50 ECU. Manipulators guess how many of the other 11 traders will have guessed that a manipulator was present. Manipulators earn 50 ECU if they guess the exact number correctly and lose 50 ECU otherwise. The purpose of the guesses in this treatment is to examine the accuracy with which market participants are able to identify manipulation, and to gauge the manipulators' perceptions of how easily market participants can identify manipulation.

Treatment 3 simulates possible manipulation with a regulator by introducing a penalty for manipulators that are detected. A regulator typically detects manipulation in one of two ways: (i) price and volume movements trigger alerts in automated surveillance systems and subjective evaluation of the alerts by a human provides grounds to believe manipulation has occurred; or (ii) market participants bring manipulation to the attention of the regulator via complaints. We use the consensus opinion of ordinary traders as a proxy for detection by a regulator. A manipulator that chooses to trade is 'detected' if eight or more of the other 11 traders (approximately three quarters) guess

[3] Although in practice manipulation conducted with the intent of decreasing the closing price also exists, it is considerably less common than increasing the closing price. In all of the closing price manipulation cases prosecuted by the US and Canadian regulators between 1996 and 2009 none involve attempts at decreasing closing prices. We believe downward closing price manipulation has similar effects on markets but leave the examination of this to future research.

that the manipulator traded, and evades 'detection' otherwise. This is a reasonable proxy for detection by a regulator because traders, in making their guesses about manipulation, observe similar information to what regulators use in market surveillance, for example, trader IDs, orders, trade prices and volumes, both graphically and in tabulated form. Furthermore, the larger the number of market participants that believe manipulation has occurred the greater the likelihood that the regulator would receive a complaint.

In each period of Treatment 3 a randomly selected trader assumes the role of manipulator. Manipulators start with the same endowment as other traders (including the clue about *V*) and choose whether or not to trade, given knowledge of the following payoffs. Undetected manipulators receive a manipulation profit of $15(P_{closing} - P_{median})$ and detected manipulators receive a detection penalty of negative the manipulation profit. In addition to the manipulation profit or detection penalty (which is zero if the manipulator does not trade) manipulators also receive 250 ECU to make their average payoffs close to those of the ordinary traders.

A rational potential manipulator in the Becker (1968) sense decides whether or not to manipulate by weighing up the gains and potential penalties from manipulation, weighting outcomes by their probabilities. Potential manipulators differ in the gains and penalties they face, their beliefs about the detection probability, and their degree of risk aversion, making manipulation an attractive option for some but not for others. We set the experimental parameters that define the manipulator's expected payoff (the multiplier of 15 and the threshold number of eight guesses for 'detection') to replicate this feature of real markets, i.e., we make manipulation attractive enough such that some, but not all subjects choose to manipulate. We verify that this is the case during pilot sessions. Other than this consideration, the choice of the manipulator's payoff parameters is somewhat arbitrary, particularly given that detection probabilities in real stock markets, penalties and payoffs are generally not known.

At the end of each period, ordinary traders and the manipulator submit guesses as in Treatment 2. In addition to allowing us to examine the ability for market participants to identify manipulation (as in Treatment 2), the guesses in this treatment also determine whether a manipulator that chooses to trade is 'detected'. Table 1 contains a summary of the payoffs from trading and guessing in each of the treatments.

< TABLE 1 HERE >

Subjects trade using computer terminals running a trading simulator (Rotman Interactive Trader) that allows them to place market and limit orders.[4] Subjects are able to see the full order book, a list and chart of trade prices and volumes and a countdown of the time remaining to the end of the period. Conversion between stocks and cash occurs instantaneously after a trade and there are no brokerage costs, short selling or margin buying. The prohibition of short selling and margin buying simply constrains the buying and selling power of the traders (including the manipulator) to the supply of stocks and cash set by the initial endowments. To avoid biasing the prices up or down, we set the initial endowments of stock and cash such that buying and selling power are approximately equal. Subjects are not allowed to communicate with one another and are aware of the payoffs that each type of participant faces. The asset values, V, clues and the manipulator allocations are randomly drawn prior to the study and the ordering kept the same for each session, as detailed in Table 2. The instructions provided to subjects consist of a core set common to all treatments, with additional instructions added for Treatments 2 and 3.[5]

< TABLE 2 HERE >

Each experimental session takes approximately two hours. At the end of a session subjects are ranked in order of their total payoff pools. The highest ranked subject receives $45; the second and third receive $40 each; the next two receive $35 each and so on down to the lowest ranked subject who receives $15. This payout method, which is similar to the method used by Bloomfield and O'Hara (1999), has the

[4] A screenshot of the trading interface is available from the authors upon request.

[5] The instructions are available from the authors upon request.

advantage that it ensures the average payoffs ($30 per subject) are equal across the three treatments and guarantees that the subjects receive at least $15. A potential downside of this method is that low-ranked subjects might be encouraged to use high-risk strategies in an attempt to increase their rank because they face limited downside risk from such actions. To reduce the potential for such effects we do not inform subjects of their rank or their cumulative payoff until the experimental session has ended. This makes it very difficult for a subject to have a sense of their relative performance after several rounds. We also analyze whether subjects' decisions and trading behavior are affected by their past performance. We find that past changes in rank, past earnings, or being the lowest ranked subject, do not have a significant effect on the decision to manipulate, the aggressiveness of orders or the level of trading activity, indicating that there is no evidence of the payoff method influencing behavior.

We conduct eight sessions; two sessions in Treatments 1 and 3 and four sessions in Treatment 2. We run twice as many sessions in Treatment 2 because Treatment 2 consists of two sub-treatments (periods that have a manipulator and periods that do not). With 16 trading periods in each experimental session we have 32 trading periods in Treatments 1, Treatment 3 and each of the sub-treatments of Treatment 2. We collect data on all trades and orders including prices, volumes, trade/order direction, trade initiator, trader IDs and timestamps, as well as snapshots of the full order book at five-second intervals. Subjects are not allowed to participate in more than one session so in total we recruit 96 subjects. The subjects are undergraduate and graduate students at a university business school.

4. Analysis

First, we analyze the effects of closing price manipulation on price accuracy and liquidity. Next, we characterize the trading strategies used by manipulators with and without a regulator and examine how manipulation affects the behavior of ordinary traders. Finally, we assess the ability of market participants to identify manipulation, and conduct some robustness tests.

Throughout most of our analysis we split Treatment 2 into its two sub-treatments, 2a and 2b, according to whether a manipulator is present. We refer to Treatments 1, 2a, 2b and 3 as the 'control' treatment, 'possible manipulation', 'manipulation', and 'possible manipulation with a regulator', respectively.

4.1 Effects on price accuracy

Figure 1 plots the average absolute price error (the absolute of the difference between trade price and fundamental asset value, *V*) at ten-second intervals within a trading period, for each treatment. Average price error decreases through the course of a trading period as a result of price discovery. Our experimental market gradually incorporates information into the price – a feature consistent with behavior observed on equity markets. Price error increases sharply in the last 20 seconds of trading in the presence of manipulation (Treatment 2b), but not in any of the other treatments.

< FIGURE 1 HERE >

We formally test manipulation's effects on price accuracy using a linear mixed effects model, similar to the models used in Hanson et al. (2006):

$$\begin{aligned}
\left|price_{ijk} - V_j\right| &= (\alpha + \alpha_i + \alpha_{ij}) + (\beta_1 + \beta_{1i})possible_{ij} + (\beta_2 + \beta_{2i})manipulation_{ij} \\
&+ (\beta_3 + \beta_{3i})regulator_i + (\beta_4 + \beta_{4i})V20_j + (\beta_5 + \beta_{5i})V80_j + (\beta_6 + \beta_{6i})period_j \\
&+ (\beta_7 + \beta_{7i})interval_k + (\beta_8 + \beta_{8i})interval_k^2 + (\beta_9 + \beta_{9i})last_k + (\beta_{10} + \beta_{10i})last_k \times possible_{ij} \\
&+ (\beta_{11} + \beta_{11i})last_k \times manipulation_{ij} + (\beta_{12} + \beta_{12i})last_k \times regulator_i + \varepsilon_{ijk}
\end{aligned} \tag{1}$$

$Price_{ijk}$ is the price of the trade immediately prior to the end of the k^{th} ten-second interval in period j of session i. $Possible_{ij}$, $manipulation_{ij}$ and $regulator_i$ are indicator variables that take the value of 1 in Treatment 2a, 2b or 3, respectively. $V20_j$ and $V80_j$ are indicator variables that take the value of 1 if $V = 20$ and $V = 80$, respectively. $Period_j$ is the trading period number within the experimental session, which takes values from 1 to 16. $Interval_k$ is the ten-second interval number within a trading period, which takes values from 0 to 19. $Last_k$ is an indicator variable that takes the value of 1 for the last interval of the trading period. Parameters α_i and β_{1i} to β_{12i} are random effects for session i, and α_{ij} is a random effect for period j of session i. Random effects and the error term, ε_{ijk}, are assumed to be distributed independently and normally with a mean of zero. Consequently, this model allows composite errors to be heteroscedastic and correlated between trading periods within an experimental session and between intervals within a trading period, but assumes sessions are independent of one another.

Table 3 reports the estimated model coefficients. In contrast to Hanson et al. (2006), the results suggest that closing price manipulation has a large and detrimental ex-post effect on price accuracy. The presence of a manipulator (Treatment 2b) causes prices to be less accurate on average throughout a trading period (by 4.82 ECU) and even less accurate in the last ten seconds of the trading period (an increase of 5.49, or total of 10.3 ECU). The magnitude of this effect is economically meaningful. The end-of-period increase in absolute trade price error that is attributable to manipulation is, as a percentage of V, between 13% and 52% (for $V = 80$ and $V = 20$, respectively).

< TABLE 3 HERE >

Further, the results indicate that possible manipulation, i.e., when there is no manipulator but traders are under the belief that there may be a manipulator, does not have a significant effect on price accuracy. This suggests that closing price manipulation does not have a significant ex-ante effect on prices, but does have significant detrimental ex-post effects. This is consistent with the main theoretical prediction in Hanson and Oprea (2009).

The results also indicate that possible manipulation in the presence of a regulator, i.e., when potential manipulators face a penalty if detected, does not have a significant effect on price accuracy. As shown in the following subsections, this is partly because the risk of incurring a penalty deters some fraction of manipulators, and partly because the remaining manipulators distort prices less to avoid detection.

The coefficients of $interval_k$ and $interval_k^2$ suggests price accuracy improves (at a decreasing rate) through the course of a trading period as price discovery takes place. Price accuracy also tends to improve through the course of an experimental session as participants learn to aggregate information more accurately. Prices are significantly less accurate for $V = 20$ and $V = 80$ than when $V = 40$. This is due to the nature of the clues about V and is discussed further in the following subsection.

Our finding that closing price manipulation has a large and detrimental effect on price accuracy is not contradictory to Hanson et al. (2006), but rather, complimentary. The two studies together demonstrate that the manipulators' incentives, defined by the payoff structure, are critical in determining the effect of manipulation on prices. In our experimental market only one manipulator trades against 11 other traders, compared to six manipulators trading against six other traders in Hanson et al. (2006). This should make distorting prices more difficult for the manipulator in our experiment. However, manipulators in our experimental market also face different payoffs. Manipulators are concerned about influencing only the last trade price, not prices throughout the entire period, and their payoff is a function of only manipulation (the difference between the median and closing prices), not their cash and asset realizations as in Hanson et al. (2006). The closing price is easier to manipulate than prices throughout an entire period because the manipulator needs only to sustain a liquidity imbalance for a short time period just prior to the close. For this reason manipulators in our experiment are detrimental to price accuracy.

We do not include cash and asset realizations in the manipulator's payoff because unlike manipulation of prices throughout a trading period where the manipulator profits from trades on the manipulated market, closing price manipulators profit from contracts external to the market and often make losses on their trades in the manipulated market. The trading losses from buying stock at inflated prices and then later having to sell the stock at the natural market price are often negligible compared to the gains from the external contract. For example, when fund managers of RT Capital Inc. manipulated the closing price of Multibank NT Financial Corp. on the last trading day of February 1999, the approximate trading loss to RT Capital for buying at an inflated price was a mere \$1,200 – approximately 0.0376% of the resultant increase in the market value of their holdings in Multibank (\$3.20 million).

Similar to the manipulator's gains, consequences of the price inaccuracy caused by closing price manipulation are external to the market and not an explicit feature of our experimental design. Traders in the manipulated market in fact gain from selling stock to the manipulator at inflated prices. Losses from manipulation are incurred by contract counterparties such as investors in managed funds that pay inflated fees or buy units at inflated prices, shareholders in acquiring companies that overpay for a target, or counterparties to cash settled derivative securities that settle at distorted prices. These examples involve a redistribution of wealth, which in itself is a zero sum game. However, there are also deadweight economic losses that result from distorted asset allocation when distorted prices are used as signals (Pirrong, 1995). Further economic losses arise from decreased participation in markets and contracts. For example, counterparties in derivative contracts that can be manipulated will demand a premium, thereby reducing the use of such contracts and increasing market incompleteness.

4.2 Effects on liquidity

We use three alternative measures of liquidity: bid-ask spread, depth and volume. Spread is the difference between the best bid and best ask, as a percentage of the bid-ask midpoint. Depth is the number of shares offered or demanded in the limit order book within 20% of the bid-ask midpoint. Volume is the number of shares traded.

Figure 2 plots the liquidity variables through the course of a trading period. The patterns are generally consistent with behavior observed in equity markets (see, e.g., Cai et al., 2004). Bid-ask spreads decline through the trading period but increase at the end, depth tends to increase through the trading period at a decreasing rate and volume increases sharply at the end of the trading period. The most apparent difference between the treatments is that spreads (depth) tend to be smaller (greater) in the control treatment than in the other treatments.

< FIGURE 2 HERE >

We formally test manipulation's effects on liquidity with a linear mixed effects model, similar to the model used to examine price accuracy:

$$\begin{aligned} Y_{ij} = (\alpha + \alpha_i) + (\beta_1 + \beta_{1i}) possible_{ij} + (\beta_2 + \beta_{2i}) manipulation_{ij} + (\beta_3 + \beta_{3i}) regulator_i \\ + (\beta_4 + \beta_{4i}) V20_j + (\beta_5 + \beta_{5i}) V80_j + (\beta_6 + \beta_{6i}) period_j + \varepsilon_{ij} \end{aligned} \tag{2}$$

Y_{ij} represents the liquidity variable in period j of session i. Bid-ask spreads and depth values are averaged across the ten-second intervals within a period, similar to a time-weighted average. Volume is measured as the total number of shares traded in the period.

Table 4 reports estimated model coefficients. Bid-ask spreads are approximately eight to ten percent wider in Treatment 2 relative to the control treatment regardless of whether a manipulator is actually present. Similarly, spreads are approximately nine percent wider when manipulation is possible in the presence of a regulator (Treatment 3). These effects are statistically significant at the 5% level and meaningful relative to the grand mean spread of approximately 20% corresponding to the control treatment. Spreads are also wider for $V = 20$ and $V = 80$ than $V = 40$, and tend to decrease through the course of an experimental session. The results are consistent with the notion that spreads are wider when there is greater uncertainty about V and that manipulation, or even the mere possibility of manipulation, causes greater uncertainty.

< TABLE 4 HERE >

Fundamental values $V = 20$ and $V = 80$ cause greater uncertainty than $V = 40$ due to the nature of the clues provided to traders. An obvious initial strategy for traders with the clue $V \neq 20$ is to buy the asset at prices below 40 knowing that either $V = 40$ or $V = 80$. Similarly, for the clue $V \neq 80$ an obvious initial strategy is to sell the asset at prices above 40. Consequently, when $V = 40$ and the set of clues is $\{V \neq 20, V \neq 80\}$ there tends to be no shortage of buyers at prices up to 40 and sellers at prices down to 40, so prices converge quickly and accurately with little uncertainty. As a secondary strategy, after having inferred the clues of other traders by observing order flow, a trader may choose to post limit orders above and below V, thereby acting as a market maker and earning the spread for supplying liquidity.

In contrast, when $V = 80$, only the traders with the clue $V \neq 20$ have an obvious initial strategy – to buy at prices up to 40. The other half, with the clue $V \neq 40$, only know with certainty that either $V = 20$ or $V = 80$ and therefore have to infer which of these possibilities is true by observing other traders' order flow. Consequently, states $V = 20$ and $V = 80$ induce greater uncertainty and cause traders to set wider spreads.

The presence of manipulators that have no regard for the fundamental asset value, V, increases the probability of observing a false signal in order flow and therefore increases the chance of incorrectly inferring V. As a result, price uncertainty is greater and traders set wider spreads. Depth and volume provide similar results as spreads – manipulation and the mere possibility of manipulation significantly decrease depth and volume.

4.3 Manipulation strategy

We characterize manipulators' order types and the timing of their trades in the absence and presence of a regulator. To do this, we classify orders into four categories of aggressiveness: market orders that execute all of the depth at the best quote and at least some of the depth at the next best quote; market orders that execute at the best quote; limit orders that are at least part filled; and limit orders that are not at all filled.

Figure 3 reports a breakdown of order types submitted by manipulators and other traders in each treatment. The most striking difference is the large number of very aggressive buy orders used by manipulators in the absence of a regulator (1.65 multiple-price market orders per period per manipulator compared to 0.14 for ordinary traders). This difference is statistically significant at the 1% level using a paired t-test (t-statistic of 4.87). Analysis at the subject level indicates that the large difference is not driven by just a few individuals: 75% of subjects use more than twice the amount of aggressive buy orders when manipulating than when acting as an ordinary trader; 15% use between an equal number and twice as many; and 10% use a smaller amount of aggressive buy orders when manipulating than when acting as an ordinary trader.

< FIGURE 3 HERE >

In the presence of a regulator, manipulators use less aggressive orders. It appears that manipulators use more of the second most aggressive order type (1.40 single-price market orders per period per manipulator compared to 0.88 for ordinary traders), although the difference is not statistically significant.

Figure 4 illustrates the timing of buy and sell trades initiated by manipulators. In the absence of a regulator, manipulators tend to sell stock around the middle of a trading period to increase their buying power and then buy heavily in the last ten seconds of trading. Thirty-five percent of manipulators execute at least one buy in the last ten seconds and 35% execute more than half of their total number of buys in the last 10 seconds. In the presence of a regulator, however, the buying activity of manipulators is less intense and tends to peak earlier. Buying activity is highest in the second to last ten-second interval, as opposed to the last interval, and involves less than a quarter of the amount of trades that a manipulator uses when there is no regulator.

< FIGURE 4 HERE >

The results in this subsection indicate that in our experimental setting the introduction of a regulator reduces the intensity of manipulation. This helps explain why price accuracy is not significantly harmed by a manipulator accompanied by a regulator. However, the penalty we impose on detected manipulation in Treatment 3 also reduces the frequency of manipulation. A rational subject would decide whether or not to manipulate by comparing the expected utility of the two options. Not manipulating offers a guaranteed payoff of 250 ECU. On the other hand, choosing to manipulate offers an expected payoff that is a weighted average of the expected manipulation profit and the expected detection penalty, where the weights are determined by the probability of detection. The payoff from manipulation is variable (risky) and therefore a risk averse subject requires the expected payoff from manipulation to be higher than the guaranteed 250 ECU to entice him/her to manipulate. Subjects differ in their risk preferences as well as their perceptions of the potential gains/penalties and detection probability, and therefore some subjects choose to manipulate and others do not.

Manipulation in this experiment, on average, is more profitable than not trading: the average payoff of manipulators that choose to trade is 296 ECU (including both detected and not detected manipulators). However, 22% of the subjects given the opportunity to manipulate the market in Treatment 3 choose not to manipulate. This fraction roughly corresponds to the perceived detection probability. Twenty-four percent of manipulators in Treatment 2 (no regulator) and 25% of manipulators that choose to trade in Treatment 3 (regulator) guess that at least eight out of the other 11 traders will guess that a manipulator was present (the equivalent of being detected in Treatment 3).

4.4 Effects on ordinary traders' behavior

Hanson and Oprea (2009) report that in their microstructure model the possibility of manipulation increases liquidity due to the desire of rational traders to profitably counteract manipulation attempts. In the context of closing price manipulation, rational traders might post additional limit orders to sell stock at prices above their expectation of V. In such a strategy the rational traders hope to take advantage of manipulators' aggressive buying, and profit from selling shares to the manipulator for prices greater than what they would receive by holding the shares at the end of the period, V. Such attempts to profit from the manipulator would increase depth on the ask side of the limit order book.

To test for evidence that ordinary traders attempt to profit from manipulation we use the mixed effects model in equation 1 replacing the dependant variable with depth at the best ask price and an alternative measure, the average depth at the best three ask prices. If ordinary traders increase depth on the ask side throughout the trading period to try and profit from manipulation we would expect a significant positive coefficient on $possible_{ij}$. If ordinary traders increase depth on the ask side at the end of the trading period we would expect a significant positive coefficient on $last_k \times possible_{ij}$.

We find that possible manipulation causes an increase in depth of 1.44 shares at the best ask price in the last ten-second interval of a trading period. This increase is meaningful compared to the grand mean, α, of 2.71 shares and is statistically significant at the 10% level. However, we do not find evidence of an increase in depth at the ask throughout a trading period nor does this effect hold for average depth at the best three ask quotes. Analysis at the subject level using the submission of a limit sell order in the last half of a round as a proxy for counter-manipulation strategies reveals relatively high heterogeneity in how often subjects use such strategies. Twenty-five percent of subjects submit late limit sell orders in at least three quarters of the rounds, 56% of subjects submit such orders in at least half of the rounds and 8% of subjects submit such orders in less than one quarter of the rounds.

We conclude that there is some evidence of ordinary traders attempting to profitably counteract manipulation by offering more shares at the best ask and that these traders believe the manipulator, if present, is likely to trade in the last ten-second interval. However, the effect of this behavior is not strong enough to prevent manipulators from distorting prices, nor is it strong enough to restore the bid-ask spread and depth to the levels in the control treatment.

4.5 Ability of market participants to recognize manipulation

In this final part of our analysis, we assess the accuracy with which market participants are able to identify manipulation. The ability for market participants to identify manipulation is important in facilitating trading strategies that exploit manipulators and restore price accuracy. It is also important for the efficient functioning of the allocative role of prices because if market participants are unable to recognize when prices are distorted, biased signals will be used in resource allocation.

Table 5 reports two-way frequencies of the guesses submitted by ordinary traders to the question of whether or not a manipulator was present in the market, as well as the percentage of correct guesses. We test the null hypothesis that the percentages of correct guesses is equal to 50%, i.e., guessing ability is only as good as chance. Despite having found that manipulation has a substantial impact on prices, surprisingly, market participants have poor ability in identifying manipulation. In Treatment 2, only 53.2% of guesses are correct, only marginally better than chance. When a manipulator is present, market participants correctly identify this with an accuracy of 49.0% - no better than chance. In Treatment 3, the accuracy of guesses is higher: 59.8% overall and 64.9% when manipulation takes place.

< TABLE 5 HERE >

4.6 Robustness tests

We test the robustness of our results to using alternative measures of price accuracy and liquidity, disregarding the first four trading periods in each session to allow participants learning time and simplification of our mixed effects regression models to random intercept models by dropping the random slopes. We find that our main results are robust to these tests.

5. Discussion and conclusions

Understanding how trading strategies commonly labeled as 'manipulation' affect price accuracy and market liquidity is critical in determining whether such strategies are harmful to markets and should be illegal (Kyle and Viswanathan, 2008). We use a laboratory experiment to examine the effects of a particular and common form of manipulation – manipulation of closing prices.

Our first key result arises from contrasting the particular incentives given to manipulators in our experimental market with those in the closely related study by Hanson et al. (2006). We find that the manipulators' incentives are critical in determining the harm caused by manipulation. Consequently, different types of manipulation should be considered separately in formulating policy decisions or in conducting academic research.

Our second key finding is that closing price manipulation harms both price accuracy and liquidity in our experimental market. Even the mere possibility of manipulation decreases liquidity and increases trading costs by increasing uncertainty. These findings are particularly concerning given the many examples of market participants with incentives to manipulate closing prices and their numerous important uses. To reduce the incentives for closing price manipulation contracts can be redesigned by, for example, using the volume weighted average price (VWAP) in place of the closing price or using "manipulation-proof" measures of performance such as those suggested by Goetzmann et al. (2007) for evaluating fund managers.

A third important result is that price accuracy can be restored by imposing a credible mechanism that monitors the market and issues penalties to detected manipulators. However, the restoration of liquidity is more difficult. The decrease in price accuracy results directly from the manipulators' actions, whereas the decrease in liquidity is caused by ordinary traders' reactions to the perceived probability of manipulation. Regulation has an immediate impact on manipulators and therefore helps restore price accuracy. However, changing the behavior of ordinary traders to restore liquidity requires that market participants believe regulation will eliminate manipulation.

Our last contribution is in characterizing a typical closing price manipulation strategy and the reactions of ordinary traders. In the absence of a regulator, manipulators submit many highly aggressive buy orders in the final seconds of trading. In the presence of a regulator, manipulators trade less aggressively and earlier in a trading period, to reduce the probability of being caught. Some ordinary traders attempt to profit from manipulators by offering more shares for sale shortly before the close. Such a strategy, motivated by self-interest, offers hope to markets for attenuating the detrimental effects of manipulation and minimizing the need for regulatory intervention. However, in order for ordinary traders to successfully counter manipulation, they must be capable of identifying manipulation. In our experimental market, despite the fact that manipulators have a substantial impact on prices, market participants have great difficulty in identifying manipulation.

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Table 1
Summary of end of period trader payoffs by treatment

This table summarizes the payoffs earned by manipulators and ordinary traders (all other traders) at the end of each trading period. N and C are the number of shares and amount of cash, respectively, owned at the end of the period. $V \in \{20,40,80\}$ is the payoff of each share at the end of a period. $P_{closing}$ and P_{median} are the last and median trade prices in a trading period. In Treatment 3 manipulation (defined as a manipulator choosing to trade) is 'detected' if at least eight of the other 11 traders guess that the manipulator traded and 'not detected' otherwise. Ordinary traders guess whether or not a manipulator was present and manipulators guess how many of the ordinary traders will guess that a manipulator was present. All amounts are denominated in experimental currency units.

Treatment	Trader type	Trading payoff	Guessing payoff
1	Ordinary	$NV + C$	
2	Ordinary	$NV + C$	+50 if correct, -50 if incorrect
	Manipulator	$15(P_{closing} - P_{median}) + 250$	+50 if correct, -50 if incorrect
3	Ordinary	$NV + C$	+50 if correct, -50 if incorrect
	Manipulator	$\begin{cases} 15(P_{closing} - P_{median}) + 250 & \text{if not detected} \\ -15(P_{closing} - P_{median}) + 250 & \text{if detected} \\ 250 & \text{if no trade} \end{cases}$	+50 if correct,-50 if incorrect

Table 2
Asset values, clues and manipulator allocations

V is the payoff in experimental currency for each share of the asset at the end of a trading period. The clue given to each subject is knowledge of one of the three possible values that V will certainly not take in that period. For example, Subject 1 in Period 1 is told $V \neq 20$. For each period of the three treatments, Panel B describes which subject, if any, is assigned the role of manipulator (given a different payoff schedule as described in Table 1).

Panel A: Asset values and clues

	Practice	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12	Period 13	Period 14	Period 15	Period 16
V	40	40	20	80	80	40	20	40	80	20	80	40	20	20	40	80	20
Subject 1 clue	20	20	80	40	20	80	40	20	20	80	40	80	40	80	20	40	80
Subject 2 clue	80	80	40	20	40	80	80	20	40	80	20	80	80	40	80	20	40
Subject 3 clue	20	80	80	40	40	80	40	80	20	40	40	20	40	80	80	20	80
Subject 4 clue	80	20	80	20	20	80	80	80	40	40	20	20	80	40	20	40	40
Subject 5 clue	80	20	40	40	20	80	40	80	20	80	20	20	80	40	20	40	80
Subject 6 clue	80	80	40	20	40	20	80	20	40	40	20	80	40	80	80	20	40
Subject 7 clue	20	20	80	20	20	20	80	20	40	40	40	80	40	40	80	20	80
Subject 8 clue	80	80	80	20	40	20	80	80	20	40	40	80	40	80	80	40	40
Subject 9 clue	20	80	40	40	20	80	40	20	40	40	40	20	40	80	20	20	80
Subject 10 clue	20	80	40	40	20	20	40	80	20	80	40	20	80	40	20	40	40
Subject 11 clue	20	20	40	40	40	20	40	80	20	80	20	20	80	80	20	20	80
Subject 12 clue	80	20	80	20	40	20	80	20	40	80	20	80	80	40	80	40	40

Panel B: Manipulator allocations

Treatment	Practice	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12	Period 13	Period 14	Period 15	Period 16
1	None	None	None	None	None	None	None	None	None	None	None	None	None	None	None	None	None
2	None	None	Subject 5	Subject 2	None	Subject 7	None	None	Subject 4	Subject 1	None	Subject 6	None	None	Subject 8	None	Subject 3
3	None	Subject 10	Subject 4	Subject 7	Subject 9	Subject 1	Subject 11	Subject 2	Subject 6	Subject 8	Subject 3	Subject 12	Subject 5	Subject 1	Subject 3	Subject 2	Subject 4

Table 3
Effect of manipulation on price accuracy

This table reports estimates from a linear mixed effects model with random intercepts and random slopes. The dependent variable is the absolute difference between the price of the last trade and the fundamental asset value at the end of each ten-second interval within a trading period. *Possible*, *Manipulation* and *Regulator* are indicator variables that take the value of 1 if the trading period is under Treatment 2a, 2b or 3, respectively. *V20* and *V80* are indicator variables that take the value of 1 if the fundamental asset value in that trading period is 20 or 80, respectively and *Period* is the trading period number within the experimental session, which takes values from 1 to 16. *Interval* is the number of the ten-second interval within a trading period, which takes values from 0 to 19. *Last* is an indicator variable which takes the value of 1 for the last interval of the trading period. *n* is the number of observations. Significance at the 10%, 5% and 1% levels is indicated by *, ** and ***, respectively.

Covariate	Estimate	t-statistic
Intercept	9.58***	4.43
Possible	1.81	0.81
Manipulation	4.82**	2.03
Regulator	0.97	0.41
V20	14.6***	8.95
V80	20.6***	9.48
Period	-0.29**	-2.00
Interval	-0.89***	-5.23
Interval^2	0.03***	3.50
Last	-0.21	-0.13
Last x Possible	-1.83	-0.81
Last x Manipulation	5.49	1.56
Last x Regulator	-0.39	-0.16
n	2,560	2,560

Table 4
Effect of manipulation on liquidity

This table reports estimates from a linear mixed effects model with random intercepts and random slopes. *Bid-ask spread*, *Depth* and *Volume* are the dependent variables. *Bid-ask spread* is the difference between the best ask and best bid prices divided by the bid-ask midpoint (average of the best bid and best ask) expressed as a percentage and averaged across the ten-second intervals within a trading period. *Depth* is the total number of shares demanded or offered within 20% either side of the bid-ask midpoint averaged across the ten-second intervals within a trading period. *Volume* is the number of shares traded in a trading period. *Possible*, *Manipulation* and *Regulator* are indicator variables that take the value of 1 if the trading period is under Treatment 2a, 2b or 3, respectively. *V20* and *V80* are indicator variables that take the value of 1 if the fundamental asset value in that trading period is 20 or 80, respectively and *Period* is the period number within the experimental session, which takes values from 1 to 16. *n* is the number of observations. Significance at the 10%, 5% and 1% levels is indicated by *, ** and ***, respectively, and t-statistics are reported in parentheses.

Covariate	Bid-ask spread	Depth	Volume
Intercept	20.43***	16.21***	31.36***
	(5.11)	(7.72)	(7.41)
Possible	8.48**	-5.19**	-12.25**
	(2.23)	(-2.26)	(-2.45)
Manipulation	10.41**	-5.33**	-3.84
	(2.46)	(-2.22)	(-0.74)
Regulator	9.34**	-3.82	-5.53
	(2.41)	(-1.51)	(-0.51)
V20	19.51***	-7.70***	9.15***
	(5.68)	(-6.22)	(3.31)
V80	14.81***	-5.69***	12.67***
	(4.41)	(-4.33)	(4.26)
Period	-1.38***	0.35***	0.19
	(-4.57)	(3.18)	(0.48)
n	128	128	128

Table 5
Ability of traders to identify manipulation

Two-way frequency tables of state (whether a manipulator was present in the market or not) and traders' guesses of whether a manipulator was present or not. *% Correct* is the percentage of correct guesses. Significance at the 10%, 5% and 1% levels is indicated by *, ** and ***, respectively, for two-sided binomial proportion tests with the null hypothesis that *% Correct* equals 0.5, i.e., the accuracy of guesses is not different from chance.

Panel A: Without regulator (Treatment 2)				
	Guess			
State	No manipulator	Manipulator	Total	% Correct
No manipulator	214	161	375	57.1***
Manipulator	175	168	343	49.0
Total	389	329	718	
% Correct	55.0**	51.1		53.2*
Panel B: With regulator (Treatment 3)				
	Guess			
State	No manipulator	Manipulator	Total	% Correct
No manipulator	30	42	72	41.7
Manipulator	92	169	261	64.8***
Total	122	211	333	
% Correct	24.6***	80.1***		59.8***

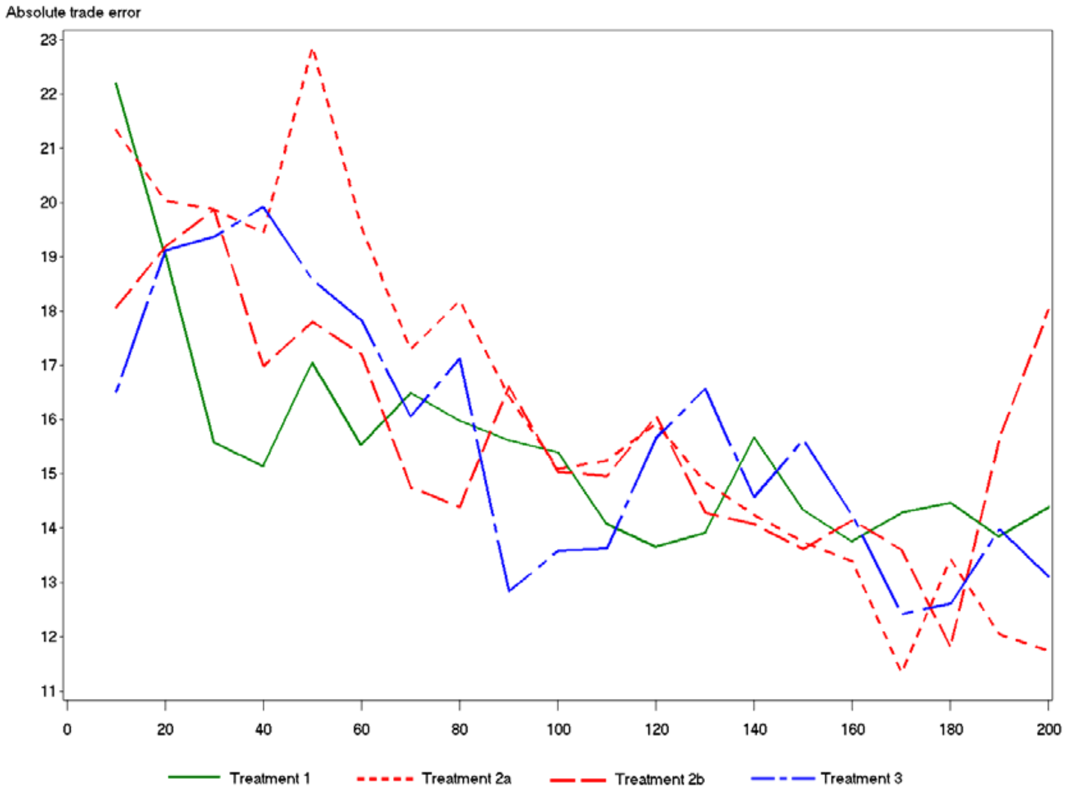


Figure 1. Average absolute pricing errors within a trading period. This figure plots the average (by treatment) of the absolute pricing error at the end of each ten-second interval within a trading period. Absolute pricing error is calculated as the absolute difference between the price of the trade immediately prior to the end of a ten-second interval and the fundamental asset value. The horizontal axis measures time (in seconds).

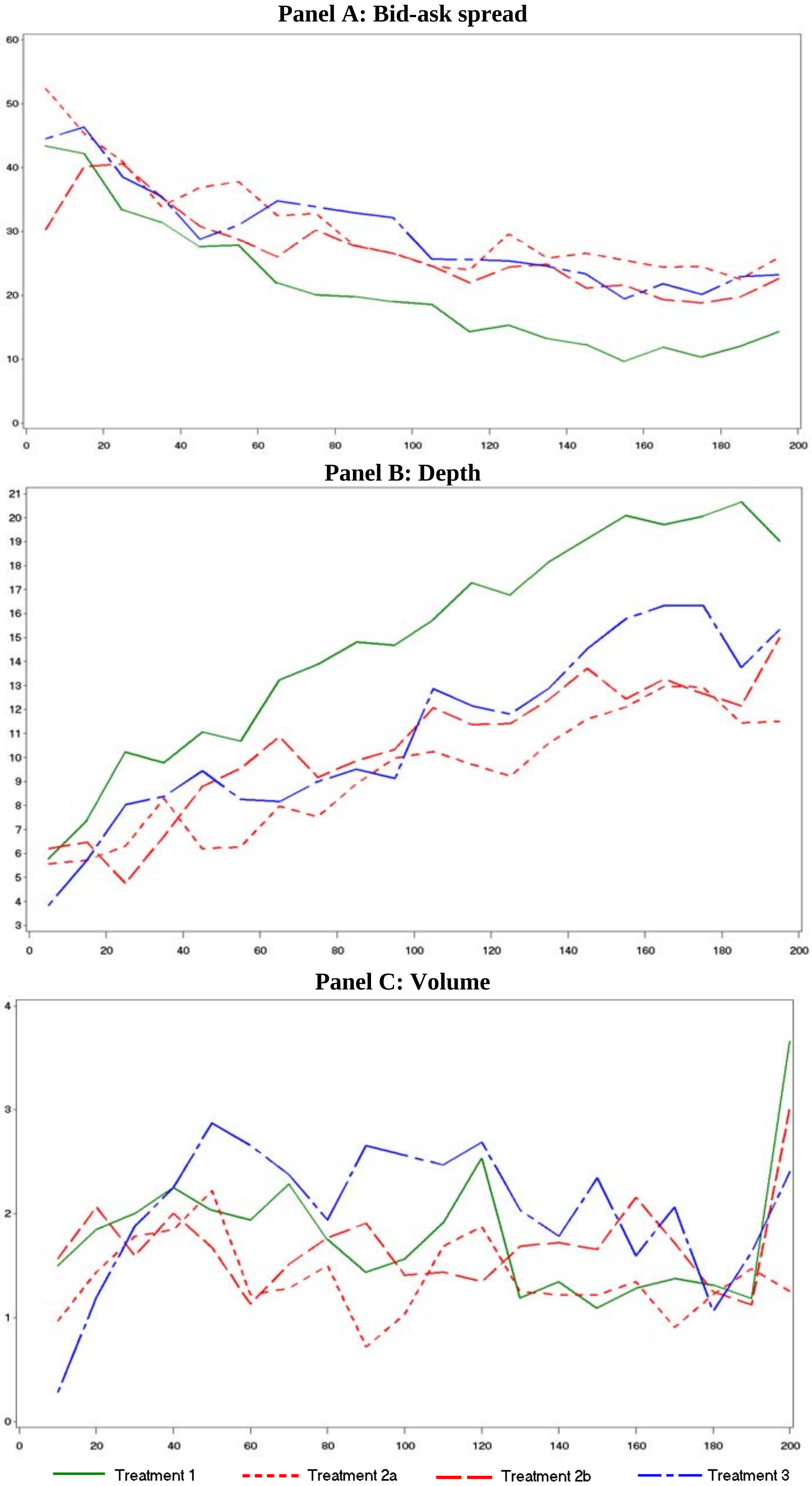


Figure 2. Evolution of liquidity variables. This figure plots average bid-ask spread (difference between the best bid and best ask as a percentage of the bid-ask midpoint), depth (total number of shares demanded or offered within 20% either side of the bid-ask midpoint) and volume (number of shares traded in each ten-second interval) within a trading period for each of the treatments. The horizontal axis measures time (in seconds).

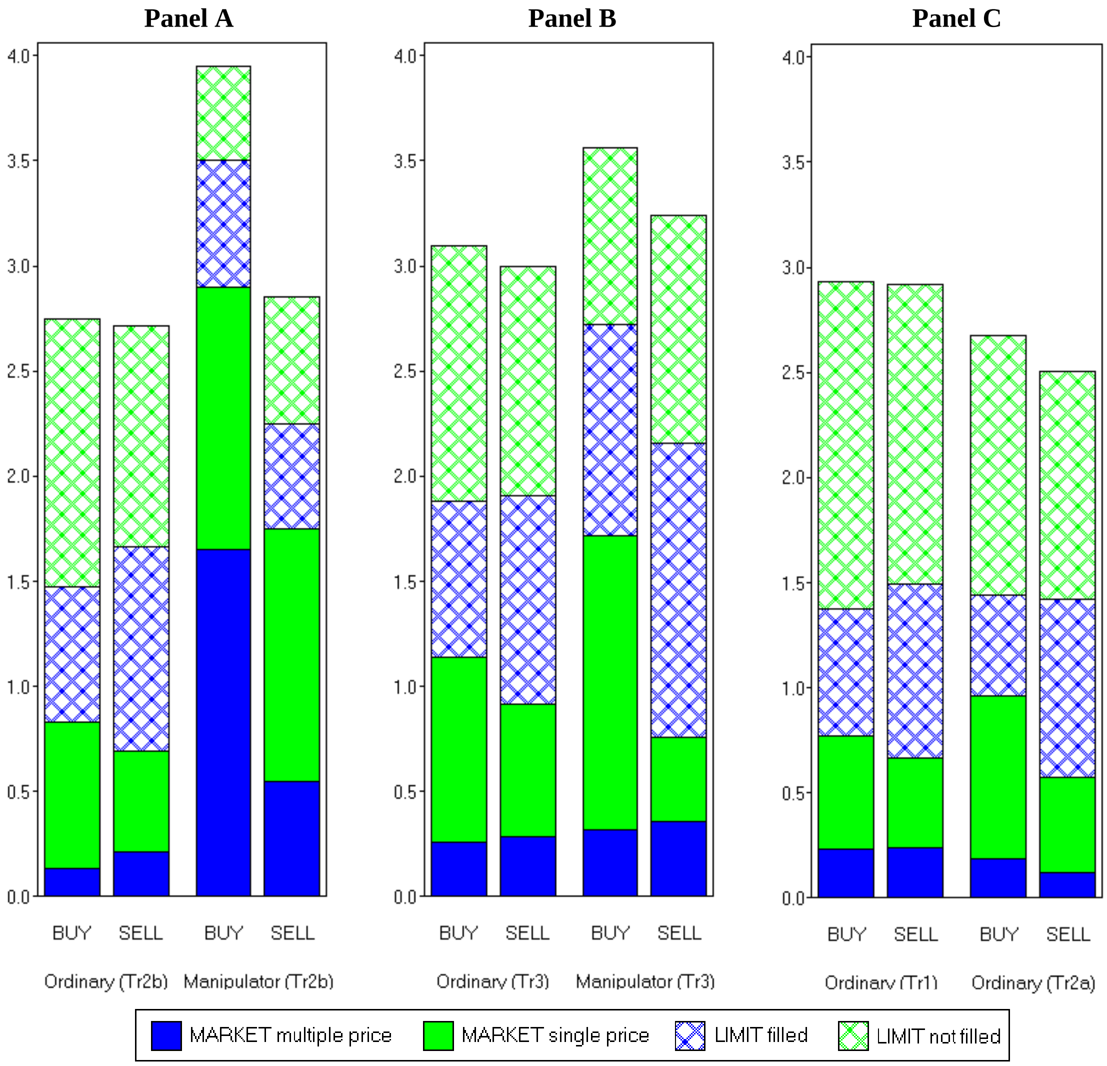
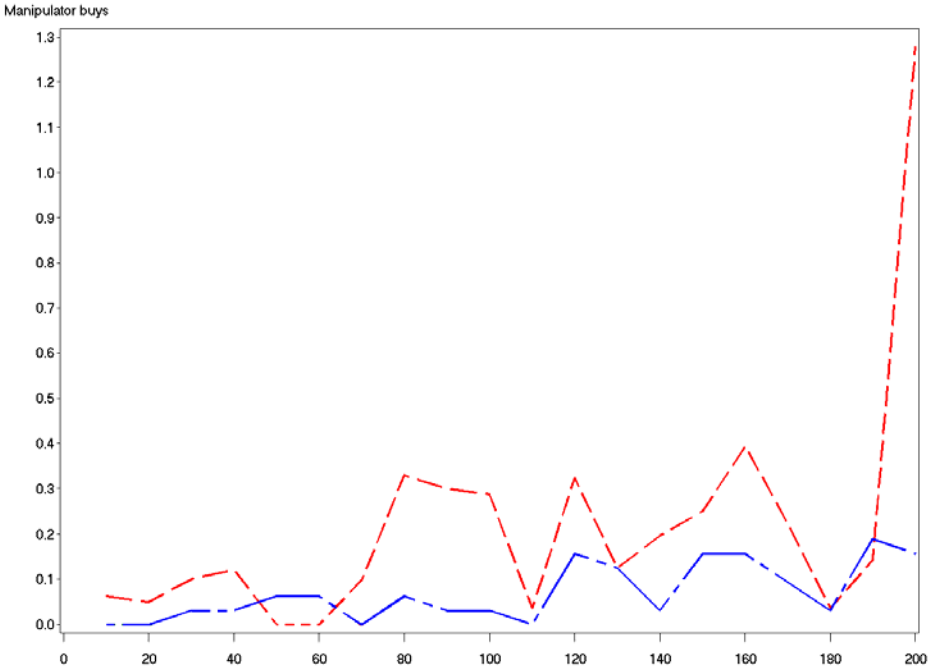


Figure 3. Order types used by manipulators and ordinary traders. This figure shows the average number of various types of order, per trader, per trading period. Panel A compares the orders of non-manipulators (*Ordinary*) with those of manipulators (*Manipulator*) in Treatment 2b (manipulation without a regulator). Panel B compares the orders of non-manipulators with those of manipulators in Treatment 3 (possible manipulation with a regulator). Panel C compares the orders of non-manipulators in Treatments 1 and 2a (control and possible manipulation). *MARKET multiple price* and *MARKET single price* are orders that execute instantaneously (either market orders or marketable limit orders) at more than one price level (cause price impact), and only one price level, respectively. *LIMIT filled* and *LIMIT not filled* are limit orders that are at least part filled, and not at all filled, respectively. For Treatment 3 we have only included trading periods in which the manipulator chose to trade to allow comparison between manipulators and other traders.

Panel A: Buys



Panel B: Sells

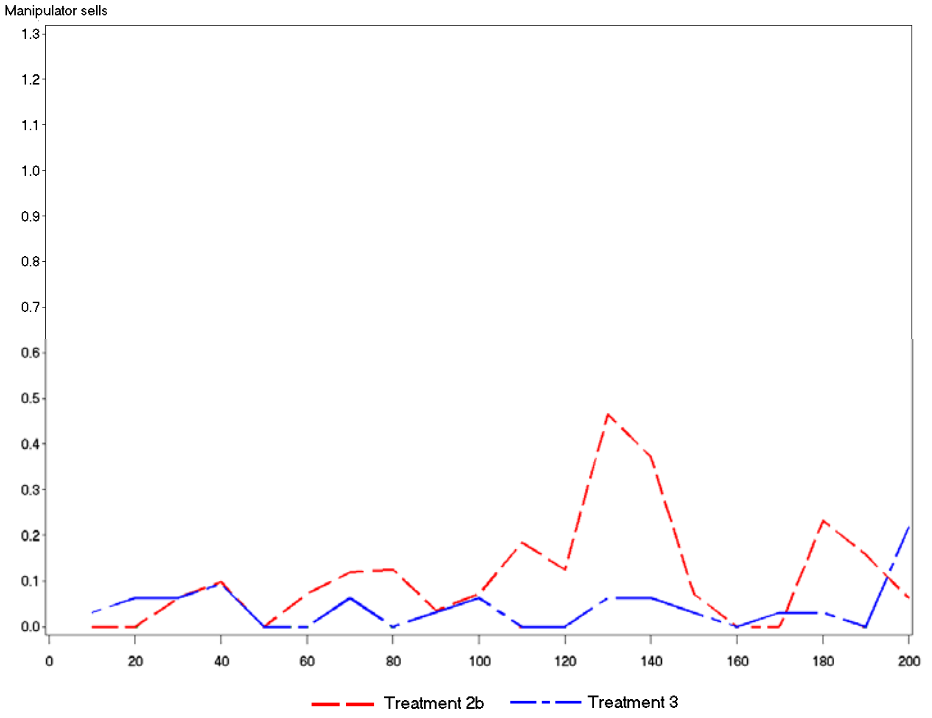


Figure 4. Manipulator buying and selling activity within a trading period. This figure plots the average number (by treatment) of buys (Panel A) and sells (Panel B) initiated by the manipulator in each ten-second interval within a trading period. The horizontal axis measures time (in seconds). For Treatment 3 we have only included trading periods in which the manipulator chose to trade to allow comparison across the two treatments.